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BOTANY.—*Naming molds.*[1] CHARLES THOM, U. S. Bureau of Plant Industry.

I owe an acknowledgment to this Society for rehabilitation as a botanist. The office I am laying down tonight was my first elective position in a botanical society. I gave up a graduate assistantship under Atkinson at Cornell to turn dairyman in 1904, and that spelled heresy to some. In that period, I once joined the crowd outside the dining room door at the Annual Dinner for all Botanists. Some one presented me to one of the elect who looked at me rather sharply and said, "I don't remember any Thom as a botanist." I replied, "All right, call me a dairyman." He answered, "Oh, then I do know you!" I never found out who he was and have always wished I had made sure of his name. I should like to meet him right now.

When I left Cornell, I was assigned to the mycological phases of producing certain varieties of cheese already recognized as ripened by molds. My knowledge of molds was vague—I was superficially acquainted with a few Mucorini, and with one or two bright-colored Aspergilli; I was vaguely conscious of the general appearance of *Penicillium* and a few more of the common genera. I knew nothing at all of the technological task that I had acquired. Professor Atkinson had written the recommendation. He was frank about it; he assured the appointing power that he knew nothing about the project, neither did Thom, but that he had more work already than he could do, whereas Thom needed the job and had brains enough to fill it. No superlatives appeared (I have read the letter). I thanked him and reported for duty. You will readily understand that you are attending a kind of confessional or experience meeting where the confessor has spent some 35 years working with molds.

AN INDUSTRIAL MYCOLOGIST

Thus I became an industrial mycologist. I entered a field in which existing mycological literature was mostly useless and in which the be-

[1] Address of the retiring president of the Botanical Society of Washington, December 5, 1939. Received December 6, 1939.

ginner was left to feel his way among materials, factory processes, ripening conditions, and biochemical aims, all unknown to him and only vaguely known to his fellow workers who were dairymen, bacteriologists, and chemists. In some groups of botanists, my status as apostate was quickly evident. It was several years before I dared to face a botanical club, point to a distinguished ecologist in the room, and say, "I am an ecologist—just as much as my friend over there. The only difference is that I make the environment for my organism while he goes out and hunts for his." Academically, then, an industrial mycologist must be an experimental ecologist, and the user of ecological studies quickly learns that sound taxonomy is the essential background of ecology. Failure in the correctness of identification of the components of a plant formation in the field destroys the value of a report.

Nevertheless, as a beginner I was about in the fix of the rookie cavalryman who had never learned to ride but reported to the top-sergeant for training in horsemanship. That hard-boiled individual blurted out, "What? Never been on a horse before! Fine! Here is a horse that's never been ridden. You two may begin together."

Professor Atkinson was right—none of us had more than the vaguest idea of the task before us. When we went to the literature it paralleled closely a fellow-worker's characterization of the German literature about sauerkraut—"it was very extensive and not worth reading." Many cheese-ripening practices were described in countries of origin as "rule-of-thumb" procedures in which climatic or other factors supplied conditions often not defined even in the worker's mind, but actually necessary to success. However inadequate the available technological descriptions, the mycology was worse.

The molds present in the cheese industry were not difficult to isolate. Rigid preliminary survey did not leave many doubts as to which were significant. Verification involved technical problems that required years of experiment after the organisms were recognized. But when I started to find out what was already known about those molds I was in trouble.

My connection with the taxonomy of saprophytic hyphomycetes began at that point and has since led me in many directions. Some of this experience will be discussed here. If at any point I may seem to generalize, please remember that the title of this paper is just "Naming Molds"—not "Systematic Mycology"—however widely I might be tempted to apply my ideas.

Why give Latin names to those wretched little molds? I can not

answer, fundamentally, but I can make the trite observation that the human mind works that way. The objects one meets get named. If we are to understand each other then, we must be able to find out what the names used by our fellows mean. Thus far the reasoning is purely practical. For illustration, I once ran across a doctor of philosophy to whom the word butter meant apple butter; if you meant the stuff made from cow's milk, it was necessary to call it cow's butter to make him understand. And *he* took a job in the dairy! A name then, to be useful, must be an *accepted* designation for a very definite thing.

Mold information, whatever its value, is indexed under the Latin names of the molds themselves. And sometimes it is not very dependable. For illustration, one very up-to-date practical and modern journal in a perfectly proper technical article, published a new species name and description a few years ago for a mold so well known among culture men that nobody but the author and the editorial committee could possibly have failed to have met it before. Since it was new to that writer, he had burst into print with his one and only new species. Judged by the tables of contents, that journal puts no limitation upon the rotten practice of the "discovery" of an organism new to the "one-project worker" who thereupon prepares a description based upon his own lack of contact with the literature of the group and sends it forth to plague all subsequent students with the addition of another synonym, or, upon the equally obnoxious practice of collecting all the miscellaneous organisms occurring in connection with a special problem, labeling as new species all those that the sender does not know and sending them to a specialist to identify, always reserving the right to describe any organism that the specialist verifies as new. Such men, not being mycologists, escape the ban.

Let us get back to our cheese molds. The technological writers had copied Latin binomials from the books, with confidence, and added the describer's name in each case. *Penicillium glaucum* Link, *P. candidum* Link, *P. album* Preuss, and several other molds were listed without question marks. When I searched the cheese literature to find out how they had sorted out names and fungi, I was driven to believe they did it by Fisher's method—"at random."

Then I tried the mycological literature to see what the names had originally meant. I worked paper by paper back to Preuss (1851), then back to Link (1809). I went on back to Micheli (1729). Many believe that Micheli intended *Penicillia* by one of his figures, but it is too doubtful to trouble us now.

PENICILLIUM

There is no question about Link's generic idea of *Penicillium*, but there is no evidence as to which actual species he had when he described either *P. glaucum* or *P. candidum*. One man's guess is as good as another's—or as Link's (yes, or Brefeld's), for that matter. Link did not know his *P. glaucum* from any one of 100 green molds. Fifteen years later he put the whole green lot together and called them all *P. glaucum*, which was designated "the common green mold" in the fourth edition of the *Species plantarum* (1824). And the idea crops up yet, after more than 100 years!

Occasionally some one raised a doubt about a universally distributed green mold that grows upon and in everything, but the name was convenient; it satisfied the pedantic requirement for a Latin binomial to be applied to material that people were not willing to study. All local fungous floras report it. The popular writers accepted *P. glaucum* as *the green mold;* chemists took it up and tested "its" activity against every kind of substratum and reagent. With probably a hundred green species to pick from, *at random*, each was able to expand the range of biochemical activity reported. Naturally with different agents, contradictions crept in and raised controversy between individuals, but the popularity of *P. glaucum* was not abated—the mistake was always charged against the worker. *Penicillium glaucum* was sacred to the shades of Link and Brefeld.

A LIVE PROBLEM

I might go on and tell more of the story of nomenclature in *Penicillium* and *Aspergillus*, but all that has been published—and the indexes are good. Suppose I shift to a problem of nomenclature that is worrying a whole group of men today, and hang a general discussion around it.

These men work with human mycotic diseases; they have had experience in the great laboratories of the world; they have excellent instruments and refined technique; they have access to literature. From a series of rather horrible lesions on perhaps 60 patients living in widely separated places, they have isolated a number of strains of mold with certain characters in common and differences that offer a chance for individual judgment in classification. Between 1915 and 1939 these cultures have been assigned to about a dozen genera even though it is doubtful if there are really more than three species. These men disagree among themselves. I have been asked several times to express an opinion as to which is the proper name, but I hardly expect

any of them to accept my conclusion. The real question, however, is: Why is there so much disagreement when the descriptive data are not only readily established but in fact generally conceded?

A sketch of the nomenclatorial situation is necessary.

In 1915, a medical worker in Boston described a skin disease but without very definitely naming it. From the lesions studied he isolated a fungus that he decided belonged to an undescribed genus and species: *Phialophora verrucosa* Medlar.

In 1920, A. Pedroso and J. M. Gomes, working in São Paulo, isolated a similar organism from cases of Chromoblastomycosis. They accepted the same name, *P. verrucosa*, which has since been commonly cited as originated by Thaxter.

In 1921, Brumpt decided that the São Paulo fungus was not Medlar's organism but another species he called *Trichosporium pedrosoi* Brumpt. (Brumpt worked for a period in Brazil about that time.)

In 1922, Brumpt, having restudied the old literature, resurrected Bonorden's generic name, *Hormodendrum*, and changed the name to *H. pedrosoi.*

In 1922, Terra, Torres, da Fonseca, and Areo de Leao, in Rio de Janeiro, called the same organism *Acrotheca pedrosoi* (Brumpt) T. T. de F. and L.

In 1928, Ota distributed material under the name *Trichosporium pedrosianum* but later decided not to publish that species name.

In 1929, Langeron (Ota and Langeron collaborated about that time) again assigned Ota's mold to *Trichosporium pedrosoi* (Brumpt, 1921).

1930, da Fonseca and Areo de Leao again published the name *Acrotheca pedrosoi.*

In 1935, Dodge resurrected the name *Gomphinaria* from Preuss's 1851 paper and moved the species to that genus.

In 1936, Negroni proposed another new name, *Fonsecaea*, calling the fungus *F. pedrosoi* (Brumpt) Negroni.

In 1937, Moore and Almeida, after collecting and comparing strains, added three more generic names for the variations encountered, basing the usage upon the presence and combinations of spore-bearing structures. These names are *Botrytoides*, *Hormodendroides*, and *Phialoconidiophora.*

In 1939, L. Briceno-Irragorry proposed another generic name, *Carrionia*, with *C. pedrosoi* (Brumpt) as its type species, arguing that this genus should include the organism of Chromoblastomycosis in South America.

CADOPHORA

In the field of forest pathology, Lagerberg, Lundberg, and Melin (1928) found species with the sterigmatic cups, which characterize the genus *Phialophora*, upon woody materials both in America and Sweden. They proposed the generic name *Cadophora* for those forms without recognizing their essential identity with *Phialophora.* More recently morphological and serological comparison of materials from human and forest sources in culture (Conant, Martin) supports the

identity of these genera; hence *Cadophora*, 1928, gives way to *Phialophora*, 1915. From the epidemiological point of view, however, Emmons (personal communication) finds grounds for belief that human infections find their inoculation in spores or mycelium from the plant-inhabiting fungi rather than from those upon fellow humans. As far as cases have been studied, man-to-man communication seems to have been excluded; hence he holds that we must search in the field among fungi growing upon vegetation for the strains responsible for cases of Chromoblastomycosis in man.

If further field study proves that these organisms are members of species regularly found upon decomposing plant remains, occasional infection of an individual human by spores or mycelia from such plant material does not warrant the establishment of either a genus or a species for that organism as a parasite. Many of these strains have grown well for me on sterilized plant material. This supports the view that search for them by culture from field samples offers a hope for solving some of these problems. Miscellaneous observation of great numbers of colonies of this dematiaceous series in connection with soil and food microbiology shows quite general growth at 37° to 38° C.—a condition usually regarded as a prerequisite to parasitism of warm-blooded animals.

HORMODENDRUM OR CLADOSPORIUM

The identification of these fungi from human lesions as congeneric at least with saprophytes or parasites of plant material turns our quest for nomenclature back to such older names as *Hormodendrum*. Bonorden (1851) distinguished his genus *Hormodendrum* from *Cladosporium* of Link (1816) by only one tangible character. Link had reported the spores of *Cladosporium* as 2-celled. Bonorden described the spores of his genus as 1-celled and transferred to it four species described by Corda as *Penicillia* but without personally seeing any of them, then added some "*Penicillia*" described by Fresenius, making a few disparaging remarks about Fresenius (1851). Two years later (1853) he clarified his ideas of *Hormodendrum* by actually describing one from fresh material (*H. atrum*). He left little doubt as to the general morphology of his mold.

Bonorden in his discussion clearly admitted that his material may, at least in part, have already borne the name *Cladosporium*. Among mycologists familiar both with specimens from the field and with the molds in culture, the 1-celled or 2-celled condition of the conidia is found utterly unreliable; hence the identity of *Hormodendrum* with *Cladosporium* has been quite generally conceded for 50 years at least.

As a consequence, application of the rules of nomenclature relegates *Hormodendrum* to synonymy.

Although Link's description of *Cladosporium* is no more definite than the other abbreviated Latin diagnoses of that time, the general nature of his material appears to have been recognized and the information handed down through continuous usage in the mycological laboratories and collections of his own and succeeding generations of workers. In this way his concept became more definite than *Dematium herbarum* of Persoon, which he believed to be the same material.

HISTORY OF CLADOSPORIUM

Before 1816, Link had assigned such forms as *Dematium herbarum* of Persoon to *Acladium* in 1809, after presumptively satisfying himself that they should be separated from the other species left in *Dematium*. Later he must have looked at them more carefully and concluded that they should be excluded from *Acladium* (branchless) and put into a genus whose name pointed to the distinctive character, branching (clados) spore chains. The code of nomenclature we use today for these groups of fungi begins to apply the priority rule with Fries's *Systema mycologicum* (1921–1932), rather than the previous publications of Link, Fries, or Persoon. Up to that time such arbitrary changes as we find in Link, Persoon, or even Fries merely furnish background for understanding the conditions under which the usages we have today were developing. *Cladosporium* is definitely recognized by Fries; hence it is valid.

CLADOSPORIUM OR SOME SEGREGATE

If, then, our reasoning is correct, these fungi isolated from Chromoblastomycosis must be assigned to *Cladosporium* unless adequate characters are available to separate some one or more of the series into one or more other genera. Without repeating details, I have already noted that the more careful workers agree that all the strains in question are closely related, at least (Emmons and Carrion, 1936).

In seeking lines for separating the series, describers have emphasized three kinds of spore production. All agree on the observation of some strains predominantly producing the *Cladosporium* or, to use their term, *Hormodendrum* type of spore chains: i.e., a more or less complex system of branching chains in which the newest cells or spores are constantly developing on the tips of the branches. They equally agree that other strains show progressive reduction of the branching system toward the ultimate simplicity of clusters of primary spores

aggregated rather densely about the clavate ends of the fertile hyphae (suggesting *Botrytis* to Moore and Almeida). Only an occasional individual cell in these groups develops a short series of buds of the *Cladosporium* type—some of them call this the *Acrotheca* type of spore formation. The fact of progressive reduction but never complete reduction, from the complex *Cladosporium* type of branching chains toward the *Acrotheca* type, leaves the homologies definite and readily recognizable.

PHIALOPHORA MEDLAR[2]

In contrast, the third type of fruiting structure is that described for *Phialophora*. Medlar figured hyphae with black or brown walls upon which directly or on short branches, singly or clustered, basal or sterigmatic cells develop. These cells termed by him phialids have firm brown to black walls and contract abruptly near the tip into spore-producing tubes, which then abruptly flare to form cups or cupules also with heavy brown walls. Colorless, thin-walled spores develop successively within the bases of the cups and tend to adhere about the tips in more or less sticky masses or spore balls. My statement is one of observation that they develop there but not how they are formed.

This structure is known to many mycologists from its presence in other series of imperfect fungi. In some species it is reported to be functional in connection with producing the ascosporic phase of life history. Outside of appearances in lesions, the life histories of the fungi of Chromoblastomycosis are entirely unknown; hence these cupules are for the present merely additional morphology, which can be used in diagnosis. Some species or strains show them regularly, some under limited conditions, while they are not known in other strains.

In culture, Miss Margaret Church and I studied the "*Cadophora*" type of structure from decaying plant materials at least 15 years ago. I have examined an occasional culture from pathogenic sources. Finally Dr. Emmons passed me 20 cultures from his collection, including transfers of strains received from Dr. Morris Moore as representing *Phialoconidiophora*, *Hormodendrum*, and *Botrytoides*. I have kept them in petri-dish and test-tube cultures for at least two months. It requires no imagination to find colony differences perhaps justifying separation into species, but essential similarities are equally ap-

[2] Not Thaxter! Even though Medlar acknowledged consulting Thaxter, he took entire responsibility for the naming and description of his species. Citation of the species as *P. verrucosa* Thaxter is common but unauthorized by any rule.

parent. I can find no reason for separating them widely. I can not agree that they should be placed in different genera.

Then how about selecting a generic name from the available dozen and putting them into it? The objection is raised that particular names are based upon the presence of particular types of spore formation, while in the cultures these do not always satisfactorily correspond with any one of the descriptions. If the group of strains from human sources appears to be too homogeneous to place in separate genera, as I think, the priority rule settles the question without further debate. *Phialophora verrucosa* of Medlar was certainly one of the series and was described first. A generic name once established loses its etymological limitation and becomes the designation of an aggregate rather than a unit or single species. Medlar's organism would be the type species. His generic description would need emendation, but that is readily furnished. Such a separation appears to be justified by the preponderance of observations to date and should be broadly enough established to include some at least of the "*Cadophora*" series described by Lundberg, Lagerberg and Melin, and others. The student of this "*Cadophora*" series upon vegetation and in culture is sufficiently impressed with its contrast in colony and spore producing characters in comparison with the "*herbarum*" lot in *Cladosporium* to be unwilling to assign them to *Cladosporium;* hence he would choose the alternative of broadening the generic diagnosis of *Phialophora* to cover the series showing these common characters until further life history studies determine real relationships elsewhere, if any. In other words, as I see it, the well-established saprophytic series "*Cadophora*" determines the placement of the vagrant members of that series which are found here and there throughout the Western Hemisphere as the cause of Chromoblastomycosis in individual humans, each time apparently *de novo*.

EPIDEMIOLOGICAL ARGUMENT

Epidemiological isolation of these cases clearly disposes of any necessity to recognize the parasitization of the individual human as justifying the separation of the causal organism, as isolated from the patient, generically or specifically from the inoculum that produced the lesion. Each organism isolated from such a lesion is to be considered merely a stray member of a species abundant in another environment. Such an occurrence is not essentially different from growth in a petri dish, which frequently diverges in superficial characters from the colony as seen in nature. In this series, then, the imag-

inary wall between fungi growing on vegetation and as fungous parasites of man has broken down. As in many other biological situations, the idea of specificity which limited organisms to particular and narrow biochemical roles proves to be false.

BACK TO BIBLIOGRAPHIC HISTORY

Having illustrated my topic by proposing an answer to a specific question, I must now go back and discuss the broad aspects of my task—naming molds. The causes of such controversies as I have just described lie back in the history of mycology covering the past 200 years. The original mycologists were essentially microscopists—laboratory examiners of material, who applied rapidly evolving microscopic methods to the description of specimens collected by themselves or others. I am sorry to express the conviction that there are many today who go little farther in their examination than Link or Persoon even though with better microscopes and more adequate literature they can not avoid seeing more.

THE FIRST 100 YEARS

If we follow back the descriptions of molds to Micheli (1729) we find that he separated the half dozen or so molds that he called *Aspergillus* (rough headed) as yellow, white, green, black, etc. There were 2 or 3 Asperigilli, probably a *Penicillium* or two, or some Mucors among them. Color seemed to him all he needed for separation. By the beginning of the nineteenth century, Persoon and Link, with a larger series of molds to separate, had raised the requirement to 3 or 4 lines of Latin. Their figures clearly indicate that their microscopes were low in magnification, but their descriptions and figures seemed adequate to men who had only a few forms to separate but did not help the next fellow who had only a few, but a different few. Details of structure and cell arrangement were not seen and were not thought necessary.

Corda (1830–1840) went a bit farther—he had a better microscope and figured individual cells, but their origin and relationship were not even considered among his recorded data. Corda could draw nice pictures—the only trouble with them has been that no one else has ever been able to find anything like his pictures of some of these evanescent molds, however valuable his drawings may be in other groups.

By 1850 Montagne concluded that most of these old descriptions of delicate fungi were entirely uninterpretable.

These men were busy naturalists—explorers of the new domain opened up by the compound microscope. Their colleagues were clam-

oring for information. Collectors in distant lands piled the tables of Linnaeus, Persoon, Link, Fries, Berkeley, Cooke, and the rest with unknown specimens. What wonder that each described in hasty terms everything that came or matched it briefly against his predecessor's briefer description, then identified or separated it and passed it into his collection. Unfortunately, most of these specimens of the more delicate fungi kept even under the most careful management quickly dried up, separated into powder, and disappeared. Verification from type specimens is thus impossible.

CULTURE ENTERS THE CONTROVERSY

Culture did not appear in the mycological literature until the time of deBary, about the 1850's, and was not used seriously for descriptive purposes before van Tieghem in the 1860's and Brefeld in the 1870's.

Brefeld did a prodigious amount of work, but he also was an artist as well as a mycologist, so that one who studies his figures with a hand lens finds that he established an interpretation in his mind, then covered the paper with diagrammatic drawings that often tally with the idea more closely than with the material under his microscope. I am not undervaluing Brefeld as a pioneer. He did much, but as to details he left much undone, and we must not hesitate to correct mistakes incident to method and equipment.

There was a parallel development of mold culture in France in the laboratories of Raulin, van Tieghem, Bainier, Gueguen, and others. Both groups of pioneers tended to assume that every form found was new, that the number of species was small; hence fragmentary descriptions run from one line to ten in length, without enough comparative work among groups of congeneric species to develop the discrimination between fundamental and ephemeral characters.

EXPANDING THE DIAGNOSIS

Technological mycology, as far as molds are concerned, i.e., the controlled utilization of particular and definitely known molds in accomplishing biochemical processes, began to appear in the literature in the 1860's. By the 1890's, a number of such processes were fairly well recognized. Sopp and Wehmer, both students of Brefeld, made extensive studies of Penicillia and Aspergilli in connection with industrial problems. Again, the items included in an adequate description were greatly increased.

In this period (1860–1890) culture as a basis of description was deemed satisfactory if a colony was obtained by any procedure. Data

from such a colony were regarded as information accessory to the study and description of natural masses presumed to be typical but were not included in diagnosis of the species. By 1890 some efforts were made to insure the purity of the colony. In the succeeding decade (1890 to 1900) species diagnoses based on colonies grown on laboratory media began to appear, limited to saprophytic molds. The substratum upon which the colony had been originally found continued to hold the key position as "habitat," which might mean much or nothing, according to the care with which the natural situation was studied and described. Many still consider designation of the place where the original specimen was obtained to be the only proper habitat to be cited. Unfortunately the habitat-substratum very commonly means only that the first gross inoculum was found there, not that the organism isolated was specifically active upon that substratum.

DEMANDS OF "APPLIED MYCOLOGY"

Thus far the methods and descriptive practices applicable to the molds used in industrial work were not differentiated from those of general mycology. The exacting demand for molds with specific adaptability to important biochemical uses began to be felt in the 1890's. Wehmer took out patents about 1893 for the manufacture of citric acid, using *Citromyces* as the fermenting agent. By 1905, he admitted that he thought at first that there were only two species, whereas he had now found that there were not less than six and he did not know which was which. Now we know there are many more.

Industrial use calls for exact information as to the biochemical possibilities of each mold. Culture media and conditions must be described with such definiteness that experimental work can be repeated and checked by analysis. Species must be described in terms sharp enough to insure identification.

Green Aspergilli are not adequately covered by the name *Aspergillus glaucus* nor all green Penicillia by *P. glaucum*. Means must be found to make species more tangible than rough aggregates held together by one or a few vague adjectives. The number of genera and species have increased beyond the wildest dreams of the early mycologists. In the effort to produce descriptions that will insure identification, species diagnoses have become progressively detailed and complicated. An extreme case may be cited: Strains of a great group were collected for many years. In working them over the monographer first developed punctilious notes as to the culture reactions of his whole series upon about half a dozen selected media. While making

these notes, he preserved colonies of each species in alcohol. After the biochemical record was completed, a mount of each species was made with extreme care and a plate of drawings representing that species was prepared. When all the plates were finished, he prepared his Latin diagnoses by describing the structures and variations depicted in his plates! Then the accumulated mass of data was sorted out to species, pieced together, and published. This is one illustration in connection with a monograph, purely academic in character. In practical fact, there are many unrecognizable species among those described in detail within the last few years, and by all of us! WHY?

UNINTERPRETABLE DESCRIPTIONS

There are several reasons for failing to identify species from descriptions. There is little agreement as to just what characters are fundamental to genera and to species, and which are incidental variations representing direct response to environment. Great stress has has been put upon numbers and measurements of parts or details of branching systems. Large numbers of spores have been measured to the fraction of the micron, then the totals averaged or statistically analyzed to the fraction of a micron. As a result, emphasis upon unimportant details has often claimed the users' attention while the fundamental information escaped.

ESSENTIALS OF DIAGNOSIS

A safe description requires the exact identification of the culture substratum, of the biochemical effects of the culture upon that substratum, and correspondingly a series of observations of the organism itself upon that substratum.

Purity of culture is essential. The presence of bacteria, *Actinomycetes*, or other molds often alters colony characters. Different contaminants produce different alterations; hence entire elimination of other organisms is essential. Unfortunately, there are quite well-known laboratories from which cultures consistently show mites, as well as molds or bacteria. One is compelled to believe that some workers have never yet seen mites, recognized their ravages in culture, or distinguished the characteristic smell that usually betrays their presence.

More important yet, actual relations and sequences between cells are fundamental. New cells may be formed by fission: The older cell is cut into equal halves; or, they may bud out at one of a dozen places

and in one of a dozen ways. Chains of spores may arise by budding so that the newest cells are always farthest from the basal cell or sporophore. Unbranched chains commonly arise from a basal cell—always showing the distal cell as the oldest while the newest cell is attached directly to the basal or sterigmatic cell. Again, this observation is fundamental.

A description of a ripe spore mass as a mass, or, pulled to pieces and the pieces described as found, may miss completely the significant facts. Whole series of descriptions that disregard cell succession in spore formation are simply uninterpretable, except in species in which satisfactory material was preserved or which have been found identifiable from some other line of observation.

Finally, details of cell wall structure (as Jeffrey said about paleobotanical specimens) may furnish as many real clews to relationship as the orthodox observation of sporogenous masses. Whole series of Aspergilli and Penicillia show such consistent markings of the stalk wall that examination of the sporophore wall with oil immersion objectives furnishes the most useful, most general, in fact, most easily determined series character.

But the nomenclatorial sins are not all chargeable to describers. The most striking lack among users of descriptive literature is in the appreciation of the cell relations involved in whatever structures they find. They fail to give proper consideration to cell contents, cell walls, their structure, color, and markings, to cell-succession in the formation of the sporogenous tissue seen under the microscope, and to the methods of aggregation of spores shown by heads, chains, or discharging mechanism. In other words, they fail to understand that identification of a mold is not accomplished by a superficial examination with a hand lens or with the low magnifications of the microscope. Exacting observation of the detail indicated in descriptive keys is ordinarily guided by those same keys. Failure to follow out definite instructions is hardly justifiable.

In the more complicated groups, it is not just matching a culture at a glance against a list of names, but the integration of all that one can learn by painstaking examination against the literature and investigations of perhaps 100 years. Into that investigation must go a first-hand knowledge of the life history of not one or two species but whole groups of species; it must include many years of observation in the field, controlled development in the culture room, and diligent reading in the library.

The need for care in establishing one's right to use generic and specific names can not be brushed away with a wave of the hand.

I sat beside a well-known worker not long ago and heard him tell an inquirer, "Why bother about the name—select one of them, go ahead and study your organism, and let future systematists decide where your organism belongs!" That dictum may suffice for some folks! But it creates chaos when the industrial or technological scientist makes the wrong selection and puts a series of industrial or biochemical papers about genus "X" and species "Y" into the literature when he actually worked with genus "A" and species "B." Indexed that way, an error is often cited many times as a fact by workers who have no means at hand to protect themselves. This is no hypothetical dilemma. I can name instances. Men regularly ask me for cultures based on such papers. Sometimes I can guess what they want; sometimes I have no idea why they make the request. Again I am sure they selected names at random.

In direct controversion of the dictum above, I believe it safe to say that a critical cultural and microscopical study of saprophytic molds will in the vast majority of cases throw together into homogeneous groups the things that eventually prove to belong together even though exact relations among them can not always be predicted from the hyphomycete stage.

To the industrial mycologist who is confronted by a mold isolated from where you please—important or merely questionable—the problem of what to call it is not theoretical, but practical. He should be able to examine the thing before him and reach some diagnostic characters that will lead to the correct literature of the species. In other words, however unimportant naming may be, as an end in itself, the descriptive and taxonomic problem must be solved before he can reach what his predecessors and perhaps his colleagues have written about the particular thing on the table.

If you are to do technical work with a particular mold, the fundamental dictum is: Know your organism by name and relationship, know it morphologically and physiologically, macroscopically and microscopically—know it so well that if anything goes wrong, you will detect the abnormality and correct it or make an adequate record. That applies whether you are a mycologist, a pathologist, a chemist, a physiologist, or any other brand of specialist; the man who fails to know his organism thoroughly is helpless before contamination, losses, or replacements, which often destroy the value of the results.

SUMMARY

I have tried to picture to you some of the problems of the "applied mycologist" who works with the so-called "common molds." As factors in human affairs, they spoil man's food, mildew his clothes, pollute his storerooms, and even attack his body. He gets some return by eating a few of them and using others in controlled fermentations of many kinds. He can not escape from them—he must live with them. The alternative, then, is to know them—individually, that he may use or combat the single species; as groups, that he may so compare and systematize his information that each item in it may contribute to arranged and ordered systems of knowledge. In a recent memorial to a great museum specialist, his services to mankind are listed as "(1) Scientific research, (2) Nomenclatural," etc. One makes bold to say that there is fully as much reasoning exhibited in comparing, reconstructing, and classifying a fossil as in digging it out of a hillside. The one dictum that must not be forgotten is that no single item has permanent value unless it represents the closest approximation to truth that can be reached by using all the means available. Any work, to be worth while, must be a rigorous search for truth. No bypath can be permitted to lure the worker aside for fancied results. If rigorous good faith in method, in performance and in interpretation are maintained, usefulness from the results can not possibly detract from the purity of the science. It is not the "pot of gold" that pollutes; it is the method of getting it. It is true in applied mycology as everywhere else that "He that entereth not by the door into the sheepfold, but climbeth up some other way, the same is a thief and a robber." If the solution of problems in human service makes our work applied science, then let us glory in the name.